tion Prof. Dendy puts forward as to its function is quite a reasonably probable one.

GEO. E. NICHOLLS.

King's College, December 17.

Nitrogen-fixing Bacteria and Non-Leguminous Plants.

I REGRET that owing to Mr. A. D. Hall's letter of July 12 (NATURE, July 22) appearing during vacation, I had not an opportunity of seeing it until my return to college at the commencement of term. I trust that, in spite of the long delay, I may yet be allowed to reply to some of Mr. Hall's questions.

Mr. Hall commences by stating that my conclusions are based "on experiments to show that Pseudomonas, the bacterium associated with the leguminous plants, will fix more nitrogen, &c." This is not correct, for it is definitely stated in the abstract that both the Pseudomonas and the Azotobacter used for the nitrogen determination results quoted were obtained from a non-leguminous plant—the root tubercles of Cycas.

There are four non-leguminous plants possessing root tubercles which contain nitrogen-fixing organisms. In all four a species or variety of Pseudomonas is present, but in Cycas only is Pseudomonas found living outside the cortical cells, in the algal zone, and in Cycas only is Pseudomonas found in association with Azotobacter. As stated in my paper, the nitrogen determinations quoted had reference only to these organisms from Cycas, and were made in order to determine to what extent, if any, Cycas *Azotobacter* assisted Cycas *Pseudomonas* in assimilating free nitrogen.

"Pseudomonas, the bacterium associated with the leguminous plants," and the use of "a reasonably active culture" of Azotobacter, to quote Mr. Hall, had nothing to do with the determination experiments. On what grounds, then, does Mr. Hall state that "the only conclusion that could be drawn from Prof. Bottomley's figures would be that Pseudomonas injuriously affects the power of Azotobacter to fix nitrogen"? Naturally, a determination of the amount of nitrogen fixed by the Azotobacter alone was made (for Mr. Hall's information I may say that it was very small, —0·56 mgr.), but as the immediate object of the experiment was to determine the effect of Cycas *Azotobacter* on Cycas *Pseudomonas,* it was not thought necessary to quote the figures for Cycas *Azotobacter* alone, especially as this will be dealt with in a future paper.

Again, when speaking of the experiments with oats, Mr. Hall says that the mean error of ±10 per cent. found in Rothamsted experiments "would more than cover the differences observed by Prof. Bottomley's experiment with oats." The figures given for oats are:—average weight per plant, untreated, 0·42 grm.; treated, 0·74 grm.; increase, 0·32 grm., or 76 per cent. Surely Mr. Hall cannot have read the abstract carefully or he would not have stated that a mean error of ±10 per cent. more than covers an increase of 76 per cent.! As regards probable experimental error, the abstract states that the oats were "grown in sand, dressed with phosphates, potash and lime." Why, then, should Mr. Hall assume that they were grown "in soil which presumably already contains both organisms"?

As regards the field experiment with barley, duplicate samples were not taken from different parts of the untreated plot, but the total yields from treated and untreated plots were kept separate, and the treated showed an increase of 13·6 per cent. The sample for estimation of nitrogen content was taken from the bulk in each case.

The treated bulbs gave an increased yield of 18·6 per cent. They were not selected as being specially "suitable for experiments on nutrition," but to determine the effect of the mixed culture of bacteria on a totally different kind of plant from any of the other experiments. The bulbs were already planted when the experiment commenced, hence the original weight of the bulbs is not available. As, however, the bed contained 500 bulbs, and was divided into two equal halves with 250 bulbs in each, one may assume that the original weight in each half was approximately equal.

As regards the culture solution "being a considerable factor in any beneficial effect experienced," it is difficult to comprehend how 4½ grs. potassium phosphate and ½ gr. magnesium sulphate dissolved in a gallon of water for the culture solution, and this afterwards diluted with fifty gallons of water before applying, could possibly produce any appreciable effect on growing crops. Assuming that the bacteria had not used up any of the culture salts in their growth to produce the culture solution, can one imagine any benefit to plants by watering them with a solution containing 1/50,000 part of potassium phosphate and 1/450,000 part magnesium sulphate?

Mr. Hall appears to consider his questions "somewhat critical." True and fair criticism is always welcome in scientific investigation, for it is only thus that one can get at the truth of things, but misstatements of fact or misrepresentations of results exceed the bounds of criticism. So far as Mr. Hall correctly states the experiments and results given, one welcomes his criticisms.

W. B. BOTTOMLEY.

King's College, Strand, W.C.,
November 25.

IN Prof. Bottomley's paper in the Proceedings of the Royal Society I cannot read that he makes any claim that the Pseudomonas and the Azotobacter he isolated from Cycas were in any way different in kind from the usual forms of these organisms. When I used the expression "Pseudomonas, the bacterium associated with leguminous plants," it was as a sort of explanatory label for the lay reader, but if it has confused the issue at all I would still repeat my former criticism on Prof. Bottomley's basis that the Pseudomonas and Azotobacter he used are special and unlike all others. The question was if "the association gave an increased power of assimilating free nitrogen," and Prof. Bottomley's figures are:—

Control...	0·48	mgm.	nitrogen
Pseudomonas alone ...	0·91	,,	,,
Pseudomonas + Azotobacter ...	1·24	,,	,,

and I asked if the action of Azotobacter alone ought not also to be known before any answer is possible. Prof. Bottomley now tells us that Azotobacter alone fixes 0·56 mgm., so that I still conclude that Pseudomonas and Azotobacter together (1·24) are less effective than when grown separately (0·91 + 0·56), were I not more inclined to think that all the differences are within the limits of experimental error.

To pass to the experiments with plants, by an error which the context rendered sufficiently obvious I wrote "oats" instead of barley when dealing with Prof. Bottomley's first-quoted experiment with soil. The oat experiment is beside the point; it only demonstrates fixation of nitrogen by Azotobacter and Pseudomonas—a fact on which we are all agreed; but when Prof. Bottomley claims increased crop production due to inoculation with the two organisms in the open ground, we do want the means of judging what weight to attach to the results. He gives for the first barley experiment a gain of 13·6 per cent.; in barley 2 the increase cannot be estimated; the Galtonia experiment shows 18·6 per cent. increase, the parsnips 21·7 per cent. Such percentage differences are not outside the limits of error for a single plot experiment worked on a large scale with every condition in favour of accuracy; when they are differences between lots of 250 bulbs or 68 parsnips I have no hesitation in regarding them as without any significance whatever. I have just had pulled and weighed two lots of fifty successive roots of mangolds growing side by side in neighbouring rows on the same plot, inside rows perfectly similar to the eye and on a very uniform plot, yet one lot weighed 220 lb., the other lot 176 lb., a difference of 25 per cent. The assistant who gathered the roots was quite unaware of the question involved; his instructions were to begin inside the plot and take fifty consecutive roots along a row, then another fifty

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roots alongside; the difference in the weights merely represents the error of random sampling when numbers are small.

Prof. Bottomley is very scornful over my suggestion that the nutrient salts in the bacterial culture may have brought about an increase in yield, but if it is easy to represent their quantity as small to the verge of ridicule, exactly the same argument might be turned against the bacteria he added. No factor should be neglected to equalise the conditions of the controls and the treated plots. I can give Prof. Bottomley examples in which the nutrient salts accompanying a bacterial inoculation have not been negligible in their effects.

I put my questions to Prof. Bottomley because he does not seem to recognise how large an experimental error he must expect; the conclusions he reaches are of such importance as to demand a more serious body of evidence than the specially selected cases he has put before us.

A. D. HALL.

The Rothamsted Experimental Station, December 13.

Positions of Birds' Nests in Hedges.

WHAT has puzzled Lieut.-Colonel Walsh (NATURE, December 16) may be referred to the law of protective devices on the part of birds, and I may say that his facts have been long familiar to other field naturalists and myself. I do not think that the direction north-south or east-west has anything to do with the selection of the nest site. If, at this season of the year, hedges are examined, even very careful "bird boys" and men will be astonished at a much larger number of nests than they observed in summer. If an explored hedge skirts on one side a public road, an "occupation" road, or right-of-way paths, it will be generally found that the nests are on the field side of the hedge, and, therefore, when looked for from the other side, much more difficult to discover than if they were placed on the road or pathway side. In cases where the hedge divides a field from a plantation, the nests are invariably on the plantation side. I knew that very well when I was a boy "nester," and was struck with it only a few days ago on examining a long stretch of hedge-fencing from a semi-public road near my home. However, there may be some esoteric law of bird-life in what Lieut.-Colonel Walsh says. Certainly his facts are most interesting.

G. W. MURDOCH.

Woodbine House, Bentham, Yorkshire,
December 17.

RADIUM AND CANCER.

THE *Revue générale des Sciences* of November 30 contains a lengthy and important article by Dr. Louis Wickham on the therapeutic action of radium on cancer, based upon observations made on 1200 patients suffering from tumours, half of which are stated to have been malignant. Dr. Wickham himself has demonstrated recently in London and in Belfast the nature of the results he has obtained, and full reports are available in the Proceedings of the Royal Society of Medicine and in the *British Medical Journal*. Therefore there is no need to reproduce the details of the article. The illustrations in the *Revue générale des Sciences* are even more startling than those which have appeared in the English journals cited. The appearances presented before and after treatment are such as will, almost surely, carry conviction to all laymen, whether healthy or suffering from cancer, that radium can cure the disease. But Dr. Wickham does not write in a corresponding spirit of optimism. Indeed, the only note of triumph is the phrase "It is delightful to think that the whole evolution of radiotherapy (the marvellous discovery of radium by P. Curie and Mme. Curie, the construction of perfected apparatus, therapeutical applications) is almost entirely French." No one may grudge this full measure of recognition to the advances made possible in Paris, on the biological action of radium, by collaboration between laboratories of physics, chemistry, and pathology. Not the least measure of praise is due to Dr. Wickham himself, both for his initiative and for his achievements.

Persons who possess an intimate knowledge of the clinical course and pathology of cancer will be less impressed by the pictures of cured cases than by what Dr. Wickham writes and what he omits to refer to. The evidence of diagnosis and of microscopical structure is imperfect. The duration of the period of benefit after treatment, as well as the ultimate fate of the patients, are the criteria by which the success of surgery is measured; but the evidence advanced in Paris falls short of good standards in both respects. Dr. Wickham lays no claim to successful treatment of secondary deposits; he says severe cases ought only to be treated when the surgeon can do nothing, and that it is too early yet to say if radium is the means which ought always to be employed. A warning is given of the necessity for caution in appraising the value of any new treatment, and, above all, of the necessity of avoiding the risk of depriving patients of other treatment which has proved itself superior, especially of surgery.

The results obtained in Paris have attracted the attention of the world. The hopes they have aroused have awakened yet greater expectations for the future, when larger quantities of radium shall be available, and the technique better mastered. Meantime, notwithstanding Dr. Wickham's caution, the writer considers a further note of warning is necessary. All that is claimed for radium is a beneficial action when applied directly to primary growths. Secondary growths inaccessible to direct surgical removal are inaccessible to radium in consequence of the restricted penetration of the rays. Whether or not means will be devised for attacking deep secondary deposits—the very site of which it may be impossible to determine—remains to be seen. The actual injection of emanation solution has met with no success. Nor is the evidence that radium has a marked elective action for cancer tissue so strong, at present, as to arouse any great hopes from more efficacious means of flooding the body with radio-activity. In short, radium does not appear to be nature's remedy for cancer, but an empirical remedy with the same shortcomings as all other such in the case of cancer, in that the local condition alone is attacked and the constitutional conditions are unassailable.

That the body can generate powers of its own, leading to constitutional changes which enable it to deal effectively with cancer, has been abundantly demonstrated by recent experiment. In given circumstances, 100 per cent. of animals bearing transplanted tumours can cure themselves. The facts ascertained show that the natural forces of the body can cope both with secondary deposits and with primary growths. Though this process of natural cure is not, and may not speedily, be elucidated, still, it is not too sanguine an expectation to anticipate that ultimately it will be. The means for checking the ravages of cancer will be found, not by searching the surface of the earth for a vegetable remedy, nor the bowels of the earth for a mineral one, but by following the definite clue, that in the living body itself forces can be elicited which effectively combat the disease. Until that goal shall be attained, when surgery fails or is unavailable, relief may be sought, but cannot be guaranteed, by resorting to treatment with radium, the full possibilities of which are not yet developed, even in Paris.

E. F. B.

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